

## ***Interactive comment on “The Geoengineering Model Intercomparison Project Phase 6 (GeoMIP6): simulation design and preliminary results” by B. Kravitz et al.***

### **Anonymous Referee #2**

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This manuscript details the Tier 1 and Tier 2 experiments of the Geoengineering Model Intercomparison Project (GeoMIP) associated with the upcoming CMIP6 effort. The experiments are well justified and for the most part clearly documented; however, some minor unclear aspects remain. I recommend publication in GMD once these discrepancies and the other comments below have been addressed.

Specific comments:

1) Introduction: The authors use the old division of geoengineering techniques to SRM and CDR, which excludes cirrus cloud thinning. They also explicitly state that they use the term “geoengineering” to specifically refer to the broad range of proposed SRM

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techniques. However, this is not true since they also call cirrus thinning ‘geoengineering’ (e.g. in subsection 2.4). Hence the first paragraph of intro should be rewritten.

2) P. 4704, lines 18-24: It is not immediately obvious how comparison of G1ext and single forcing CFMIP experiments will help gain better understanding of the response to radiative forcing. Are the irradiance changes in CFMIP comparable in magnitude to G1? Overall, I feel that the authors expect the reader to know quite few details of the CMIP6 or associated other MIP experiments; it could be helpful to give slightly more background info.

3) P. 4706, lines 21-23: “Modeling groups that have —”. I’m not sure what this sentence means; please reformulate.

4) The difficulties in setting up G3 are mentioned a few times, but it isn’t explained what those difficulties were and why the new proposed setups are better. Either elaborate or leave mentioning earlier difficulties out.

5) Why isn’t sea salt geoengineering included in the G6 experiment family?

6) Are the cirrus clouds descriptions in most ESMS mature enough at this point to yield meaningful results from G7cirrus?

7) Subsection 2.4 (G7cirrus) should be rearranged as it is in many places difficult to follow its logic. I urge the authors to go carefully through the whole subsection, but give some pointers below:

- Third paragraph (p. 4708, line 23 –): The placement of the sentences “However, this approach could be difficult — not optical depth.” is very odd, and distracts from the fact that (presumably!) the GISS ModelE2 simulations are the previously mentioned “first attempt at representing the effect of cirrus cloud thinning”; Why can factor  $e$  “only be implemented for ice clouds with temperatures below 35 C and pressures lower than 600 hPa”?

- Fourth paragraph (p. 4709, line 4-): “not enough to achieve the goal of G7cirrus” –

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however, the goal of this experiment is not mentioned before this statement; Do the single model simulations with treatment of cloud microphysics show reduction of cirrus coverage? Shortly summarize what is the main difference between these and the GISS simulations.

- Fifth paragraph: (P. 4709, line 17-): “Ice sedimentation velocity is used —“ seems a very random comment in the context of G7cirrus description; “They found that —“ Who does “they” refer to (i.e. if it doesn’t refer to Liu et al., does it include Muri et al?)?; “lending credence our chosen method” You haven’t told yet what your chosen method is; “involving parameter perturbation” what parameter are you talking about (presumably fall speed)?

- Could Fig 4 be excluded as there is very little discussion of it in the text?

- P. 4710, line 5: “This representation is also not ideal—“ What representation? Increasing fall speed by varying amounts?

- Shortcomings of the chosen approach are scattered in several places throughout the subsection → rearrange; It could be clearer to first introduce the chosen approach, and only then discuss its benefits and shortcomings.

- P. 4711, lines 19-26: I don’t understand why this paragraph is included here (i.e. only under G7cirrus) considering that time slice simulations are performed also for all other Tier 1 experiments.

8) I assume the motivation for using RCP6.0 (instead of e.g. ScenarioMIP high forcing scenario) as a background scenario in G4SSA is that some of the required model runs have already been performed? Would it be unrealistic to rerun those simulations to obtain consistent baseline scenarios throughout GeoMIP6?

9) P. 4714, line 1: “different treatments of — aerosol microphysics” But isn’t the whole point of this experiment to eliminate the differences in aerosol microphysics?

10) P. 4714, line 2-5: I don’t doubt this sentence is true but you should elaborate on  
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how the comparison of CCMs and ESMs could reveal the important mechanisms (+ which mechanisms) or guide identification of processes in need of improvement.

11) P. 4714, line 7-10: Emissions reduced linearly to zero (or even below if lowest scenario assumes negative emissions?) during what period? Assumably 2100-2300, but this should be explicitly stated.

12) GeoMIP Testbed: Is the testbed closed, i.e. meant only for the three experiments listed or could further experiments be suggested? If so, what is the procedure to do so?

13) P. 4715, line: 17: “As was stated in Sect. 2.2—“ I cannot find this statement in Sect 2.2.

14) The goal of G6sulfur\_limits does not seem very clear to me. It is stated that it is to find out “what is the limit of achievable radiative forcing”; however, the experiment design seems to focus on whether stratospheric SRM could be used in sufficient amount to reduce forcing from high to low forcing scenario – it is very unlikely that the exact amount of sulfur to achieve this would be the \*limit\* of achievable stratospheric SRM.

15) My final comment is a non-scientific one and therefore should not affect the review process, but I’d love to hear the authors’ views on it: I seriously worry whether the shift towards policy relevancy in geoengineering experiment designs, e.g. using SRM to move from worst-case forcing scenarios to more tolerable forcing scenarios, reflects a shift in scientists’ views on geoengineering. Some years ago there used to be quite a wide consensus among those in the field that overall geoengineering is a bad idea (due to a variety of ethical, governance and natural science uncertainty issues) and should only be considered in case of climate emergencies. However, the new experiments were “designed to open the door toward possible conversations with designers of climate change scenarios”, so I am left wondering whether the community has started to consider geoengineering as a viable policy option (I find it difficult to believe that the outside world wouldn’t interpret it this way). Whether or not this is the

case, one is of course free to run whichever scenarios one likes purely for scientific interest. However, no matter how carefully we scientists try to formulate our message to the general public/policy makers/stakeholders, in the end we have relatively little control on how our results are interpreted and used. Therefore, I personally would have preferred continuation of the more idealized, and less policy-oriented scenarios also in GeoMIP6.

Technical comments:

- P. 4708, line 9: show allow → should allow?
- P. 4710, line 21: “\*a\* simple approximation similar to Muri” or the “\*the\* simple approximation of Muri”?
- P. 4712, l 17: response → responds?

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Interactive comment on Geosci. Model Dev. Discuss., 8, 4697, 2015.